ive is (1) for positive response, tartrate>citr.>SO_4>acet.>ClO_3>Cl>NO_3>J >CNS; Rb>Cs>K>Li>Na; (2) for negative response, (*a*) for alkali salts, acet.>tartr.>J>Cl>Citr.>ClO_3>CNS>SO_4>NO_3; Na>Li>K>Cs>Rb; (*b*) for salts of alkali earths, J>Br>acet.>Cl.; Mg>Ca, Ba, Sr.

The bromide and perhaps the iodide of alkali earths do not produce the positive response. For negative chemotropism, salts of alkali earths are about 10 times as effective as those of the alkalies. PORODKO believes from the relative effectiveness of ions that the negative excitation involves coagulation of cell lipoids as well as proteins, and that both the positive and negative irritability are lined up with change of condition of cell colloids. Our knowledge in this field is too limited, however, to follow the parallel in any detail.—WILLIAM CROCKER.

A theory of evolution.—Lest we forget that the method of evolution remains undemonstrated, that evolution itself is somewhat of a scientific dogma, it is desirable occasionally to have our attention called to the fact that scarcely two even of the clearest-headed of our contemporary thinkers hold the same views regarding it. Read BERGSON, BUTLER, DEVRIES, SEMON, WEISMANN. Their works are lawyers' briefs, written to uphold the importance of a single possible evolutionary cause to the greater or less neglect of all others; but they are remarkable briefs for all that. Evolution is due entirely to a directive force within the organism; evolution is due wholly to imperfections in the machinery of heredity and has no directive aim whatever; evolution is due solely to external causes; it is gradual, it is discontinuous; it is continual, it is periodic. One is often led to believe that no middle ground, no combination of methods is possible; moreover, it is often the individuals that strum continually on a few notes, who force recognition of tunes labeled conspicuously with their own personalities, by loud and persistent reiteration.

The latest writer to be enrolled as an exponent of a unique cause of evolution is LOTSY.[8] The unique cause is hybridization and the author's brief has been extraordinarily well done. As the paper is introduced by the following statement, the words "unique cause" do not exaggerate the position taken:

> Toutes les théories de l'évolution, y compris celle de Lamarck, Darwin et DeVries, s'appuient sur l'hypothèse, ou sur la prétendue preuve, qu'il existe d'une façon ou d'une autre une variabilité héréditaire. Le présent travail a pour but de prouver que cette hypothèse se base sur une erreur; qu'il n'y a pas de variabilité héréditaire, sous aucune forme, mais que les expèces sont constantes.

LOTSY does not believe that Linnean species are constant, but that forms truly homozygous are constant. His outline of the growth of evolutionary belief shows this.

A. The period when it was believed that the characters of an individual were transmitted as a whole.

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[8] LOTSY, J. P., La théorie du croisement. Arch. Néerland. Sci. Exact. et Nat. III B **2**:1–61. 1914.

a) The idea that Linnean species form a natural system.

1. Belief in the constancy of Linnean species.
2. Belief in the variability of Linnean species.

b) The idea that the elementary species of JORDAN form a natural system.

B. The period in which proof was given of the independent transmission and chance recombination of characters.

a) Belief in the variability of JORDAN'S species: the theory of mutation.

b) Belief in the constancy of JORDAN'S species: the theory of crossing.

LOTSY'S arguments are based upon the following assumptions, on each of which there is room for a difference of opinion: (1) that all characters obey the Mendelian law of heredity, (2) that acquired characters are never transmitted, (3) that homozygotes are absolutely constant in successive generations, (4) that there has been no proof of variation independent of crossing, and (5) that the variations observed after crossing are sufficient to account for evolution.

The first point is taken for granted, although there are certain to be many objections to it raised. Even the most ardent Mendelians have only gone so far as to assert the generality of Mendel's law in the sense that the transmission of many characters is controlled by some mechanism that is widespread in the animal and plant kingdoms, probably the chance apportionment of the chromosomes to either of the two daughter cells at the reduction division. The probability that there are many cases where such a mechanism is replaced by others can hardly be denied, though JENNINGS' recent investigations on paramecium indicate a generality of segregation hitherto unsuspected. Furthermore, even in cases where Mendel's law might be expected to be valid from other considerations, as in the Oenotheras, there appear to be independent or subsidiary laws at work which modify the results.

Perhaps most biologists admit that the inheritance of acquired characters has never been proved experimentally; on the other hand, it may be that it is impossible to prove that the hour hand moves in an experiment covering a second of time, as BUTLER very aptly puts it. Is it not justifiable, therefore, for experimentalists to assume non-inheritance of acquired characters as a practical working hypothesis for experimental biology, and just as permissible for evolutionists to assume their transmission over long periods of time, provided it is granted that this is a mere assumption? LOTSY applies the *negative* conclusions of experimental biology to evolution, a method that is always open to criticism.

The contention that homozygotes are absolutely constant in all succeeding generations is also disconcerting. The conclusion is drawn largely from JOHANNSEN'S work, yet JOHANNSEN believes in mutations. But even leaving out of account the sudden changes that have appeared in material apparently homozygous, there are those who believe that the pure line work does not show constancy in succeeding generations. CASTLE, for example, who does not come to a conclusion without due consideration, believes that selection

may always modify a homozygous character. The reviewer, taking a middle ground, believes that the pure line work least subject to criticism, that on self-fertilized material, does prove homozygotes to be sufficiently constant in succeeding generations to make this constancy a basis for mathematical description, but he believes it to be unbiological to assert this constancy as absolute.

As the basis of his assertion that there has been no proof of variation independent of crossing, the author notices only the work of DEVRIES. Undoubtedly there is a great deal in favor of the idea that the *Oenothera* mutants are the results of segregation from crosses, though the phenomena have not been fitted into present Mendelian concepts. But that the very fine contributions of DAVIS and HERIBERT-NILSSON on this subject have clinched the matter, as LOTSY believes, would not be asserted, I venture to say, by the authors themselves. It is pointed out very clearly that both the constructive work of GATES in defense of the *Oenothera* mutations and his criticism of HERIBERT-NILSSON are not so conclusive as that author so confidently asserts, but this is only negative evidence. Moreover, the work of MORGAN, JENNINGS, BATESON, and others on the occurrence of mutations in controlled cultures is complacently neglected.

LOTSY's own extensive work on specific crosses in the genera *Nicotiana*, *Petunia*, *Pisum*, and *Antirrhinum*, the constructive work of the paper, is exceedingly interesting, and his detailed accounts, which are in press, will be eagerly awaited. In brief, all the inter-specific crosses that he has undertaken have shown true Mendelian segregation. The conclusion of DEVRIES, drawn from the peculiar behavior of the *Oenothera* species, that inter-specific and intra-specific crosses obey different laws of heredity, is shown, therefore, not to be of general validity.—E. M. EAST.

Cecidology.—Among the very important contributions to European cecidology are HOWARD's papers on the collection in the Museum of Natural History in Paris[9] and from Western Africa,[10] all of which are taxonomic in character and well illustrated. The author uses the modern method of grouping the galls with reference to the host plants, which makes the data available to those botanists who are interested in the study of malformations of plants and in the relation of plants to other forms of life.

ROLL HOWARD[11] presents an exceptionally good paper on the anatomy of the galls on the margins of leaves. He divides these malformations into four groups; those caused (1) by hypertrophy and hyperplasia, (2) by hyperplasia,

[9] HOWARD, C., Les collections cécidologiques du laboratoire d'entomologie du muséum d'histoire naturalle de Paris: Galles de Burséracées. Marcellia **12**:57–75. 1913; also Galles d'Afrique et Asie **12**:102–117.

[10] ———, Les Galles de l'Afrique occidentale française. VI. Cécides du haut Sénégal-Niger. Marcellia **12**:76–101. 1913.

[11] HOWARD, ROLL, Recherches anatomiques sur les Cécidies foliaires marginales. Marcellia **12**:124–144. 1913.